

Interactive comment on “Anvil microphysical signatures associated with lightning-produced NO_x” by J. L. Stith et al.

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Comments from Referee 2

This is a clear, well-written paper that outlines an interesting relationship between anvil ice microphysics and lightning-NOx in thunderstorm anvils observed during the Deep Convective Clouds and Chemistry (DC3) experiment. Several case studies relating ground based radar and lightning observations to DC3 aircraft cloud particle images and trace gas observations are shown to support the argument that enhanced storm electrification is related to the production of frozen drop aggregates in the storms analyzed. In addition, one case contrasting characteristics observed in two storms with varying lightning activity was used to further elucidate the relationship between micro-

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physics and electrification. However, there are some important shortcomings of this study that should be addressed for this manuscript to be considered for publication, which are outlined below.

As outlined in the abstract, the primary conclusion presented in this study is “The abundance of frozen drop (chain) aggregates vs. individual frozen droplets in the central anvil region of the strong thunderstorms that were studied appears to be related to the degree of electrification (marked by increased lightning flash rates).” While the 6 June and 15 June case studies presented best demonstrate this potential relationship, the 25 May storm comparison presented casts significant doubt on this relationship. In particular, the weaker storm (that with lower lightning activity) shows the highest concentrations of particles identified as frozen drop aggregates of any storm observed during DC3, which the authors readily admit. An alternative argument would be that the microphysical characteristics observed (specifically the production of frozen drop aggregates) are more directly related to storm dynamics. This is evidenced by the distribution of individual frozen drops and frozen drop aggregates in the anvil and the concentration of individual drops in the weaker and stronger cells. The intensity of the updrafts (and thus the microphysical composition of the storm) would likely be related to the lightning frequency, so it is not surprising that some semblance of a relationship would be found.

Some additional analysis and/or literature review could better elucidate the roles of electrification and dynamics on the microphysical composition of these storms. I am not an expert on the formation of chain-like aggregates, but perhaps there is sufficient laboratory evidence for linkages between their rate of formation and the degree of electrification. The analysis could be improved if some additional details on storm evolution and strengthening between in situ observations and remotely sensed observations (i.e., radar, lightning) were presented. For example, many DC3 cases included samples in the anvil regions of a single storm at increasing range, which could reveal more information on the history of an individual storm and allow for more comprehen-

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sive ties between storm dynamics, lightning activity, and microphysical characteristics. In addition, cases where dual-Doppler velocities are available (such as 6 June) can be made stronger by improved linkages between the in situ and remotely sensed observations, which are somewhat vague in the current version of the manuscript. In particular use of a trajectory model and the dual-Doppler wind fields would better tie the historical convective core to the in situ observations.

Author's response to referee 2.

We thank the referee for the helpful comments on our manuscript. Specific responses to paragraph two and three are given below.

The referee brings up a worthwhile concern that is addressed in the revised manuscript, although her/his suggestion that the production of frozen drop aggregates (FDAs) might be related to storm dynamics as an alternate to their formation by electrical effects is misleading. Clearly, storm electrification is directly coupled to storm dynamics, so both processes are possible candidates. This is consistent with our conceptual model (Fig. 9 in Stith et al., 2015) and accompanying text; however we revise our description of the resulting microphysical signature as discussed below. This model allows for either electrification or riming as possible aggregation mechanisms producing the low-density FDAs observed in the anvil. (Stith et al. (2014) discuss an alternative to FDA formation by electrical forces.) Storm electrification is likely to be linked to FDA formation by either riming or electrical forces, so there is reason to expect that NO_x and FDAs are related in either case. The examples of long-chain FDAs that have been repeatedly observed in different storms (e.g. in Stith et al., 2014 and references therein), appear to provide the best morphological evidence to-date for a role for electrical forces in FDA production, but this may not be the only mechanism involved.

The referee correctly points out that the 25/26 May weak storm anvil contained high concentrations of FDAs, which was discussed in the paper, but not adequately ex-

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plained. It is noteworthy that this anvil contained not only higher concentrations of FDAs, but also higher concentrations of un-aggregated frozen droplets and higher total water content than the other storms. Thus the observations indicate a storm cell with low electrification and high water content in the anvil, which would be consistent with low precipitation efficiency. The resulting pattern of frozen droplets and FDAs in the weak anvil does not follow the same signature (droplets on the edges, FDAs more concentrated in the center) as the three other cases. However, we believe that the conceptual model, as described in Figure 9 is still a good framework for understanding the signature from the weak storm and we have now included a new section in the discussion that explains how such a pattern is likely to occur based upon the conceptual model.

In the final paragraph of the review the referee suggests more analysis through literature review, examining more aircraft passes, and the use of a trajectory model with the 6 June case, to better elucidate the roles of electrification and dynamics on the microphysical composition of these storms. We are not aware of more recent laboratory work on electrical aggregation than the studies reviewed in Connolly et al. (2005). Certainly additional laboratory work would be most helpful in understanding this phenomenon, which may turn out to be an important mechanism that may not be adequately recognized in our current understanding of storm microphysics. We agree that the use of a trajectory model together with more pass-by-pass data, especially for DC3 cases such as 6 June that have multi-Doppler data, would likely be a good avenue for further research with the DC3 data. Such a detailed analysis, looking at multiple aircraft passes, multiple trajectories, and associated remote sensing data (LMA and polarimetric radar), for even one storm, is best covered as the subject of a separate paper. With 14 figures in the current manuscript, it would be difficult to include such analyses in any sort of comprehensive manner in the present paper, without making it unduly long.

Summary of modifications to the paper.

We have modified the section on the 25/26 May case, especially in the discussion, C12007

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which is now substantially rewritten to explain the doubts raised by referee. This also resulted in a modification of the abstract in line with the changes to the paper's conclusions.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 15, 31705, 2015.

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